Promises and Perils of Mobile Voting

Anthony Fowler[1]
Harris School of Public Policy
University of Chicago
anthony.fowler@uchicago.edu

June 2019

Abstract

Voters are systematically unrepresentative of the eligible electorate. Many reforms intended to increase turnout and improve the representativeness of the voters have had underwhelming results. The ability to cast votes on a mobile device could potentially have more powerful effects since mobile voting would drastically lower the cost of voting, particular for certain underrepresented groups. In 2018, West Virginia became the first U.S. state to utilize mobile voting in a federal election, allowing it for overseas voters from 24 of its counties. I utilize this trial to assess the likely effects of mobile voting on the size and composition of the voting population. Implementing a differences-in-differences design with individual-level administrative data, I estimate that the ability to vote with a mobile device increased turnout by 3-5 percentage points, a large effect relative to other electoral reforms. At the same time, novel survey data shows that many Americans are understandably wary of online voting.

[1] I thank Tusk Philanthropies and the Chicago Harris Cyber Policy Initiative for funding. I thank Jake Braun, Mary Hanley, Deak Kersey, Sheila Nix, Nimit Sawhney, and Brittany Westfall for providing information and logistical help.

Voter turnout is often low and unequal (e.g., Verba, Scholzman, and Brady 1995; Lijphart 1997), and this can have significant electoral and policy consequences (e.g., Bechtel, Hangartner, and 2015; Fowler 2013, 2015). Practitioners have proposed many reforms for increasing turnout and reducing inequalities in participation including early voting, vote-by-mail, less restrictive absentee voting, election-day registration, youth preregistration, and motor-voter registration. While these reforms have increased participation, the substantive magnitudes of the effects are often underwhelming (see Berinsky 2005 for a review).[2] Even get-out-the-vote interventions which reliably increase turnout (Green and Gerber 2015) tend to exacerbate the differences between voters and the eligible population (Enos, Fowler, and Vavreck 2014). Aside from dramatic reforms like compulsory voting, it appears difficult to meaningfully increase the participation of underrepresented groups and reduce inequalities in participation.

One understudied reform that has the potential to meaningfully affect participation is mobile voting. If voters could cast their ballots online using only their cell phones or mobile devices, that would dramatically reduce the costs of voting, particularly for underrepresented groups, and it could have significant effects on the size and composition of the voting population. Although mobile voting might seem unrealistic to many Americans, it has been attempted in several different countries in local and federal elections, and West Virginia just became the first U.S. state to utilize mobile voting in a federal election, allowing it only for overseas residents from some of its counties.

We currently have mixed evidence on the effects of online voting on participation around the world. Several European countries abandoned internet voting after seeing that the increases in turnout were not as large as expected (see Goodman and Stokes 2018 for discussion), but we might worry that internet voting was implemented when turnout was otherwise declining. Utilizing more compelling

[2] Since Berinsky's influential review, the literature has continued to produce mixed and often underwhelming estimates of the effectiveness of electoral reforms. See, for example, Kousser and Mullin (2007), Burden et al. (2014), and Fowler (2017).

differences-in-differences designs, Goodman and Stokes (2018) find that online voting in local elections in Ontario increased participation by 3.5 percentage points, and Germann and Serdult (2017) detect little effect of internet voting relative to mail voting in Swiss referendums. We have virtually no evidence on the effects of internet voting in the U.S. because until recently, internet voting had never been attempted in a major U.S. election.

In this paper, I first present new survey data on U.S. public opinion about voting technology. Americans report being less willing to utilize online voting compared to other voting technologies, and this is particularly pronounced among older, poorer, and less educated Americans. I also find that informing people that online voting will be secured by blockchain technology has a counterintuitive negative effect on their willingness to vote online, perhaps because this leads people to think more about security concerns. Overall, the survey evidence suggests that Americans are quite wary of internet voting.

To see what actually happens when Americans have the opportunity to vote online, I study West Virginia's trial with mobile voting in 2018. I utilize administrative data and take advantage of the fact that mobile voting was only available for overseas residents from some of West Virginia's counties, allowing me to implement a differences-in-differences design. Among people likely living overseas, I find that having mobile voting as an option makes people 6-9 percentage points more likely to request a ballot (mobile or otherwise) by submitting a Federal Post Card Application, and it subsequently makes then 3-5 percentage points more likely to cast a ballot. A back-of-the-envelope calculation suggests that approximately half the people casting a ballot with the mobile app would not have otherwise voted if mobile voting was not an option. Mobile voting can increase turnout, and these estimated effects are greater than the effects of most other electoral reforms. Furthermore, because the West Virginia trial required individuals to first request mobile voting by submitting a mail application, the effects could potentially be greater if it were implemented in a more convenient way.

At the same time, public wariness of online voting should be carefully considered before rolling out mobile voting more broadly.

Public Opinion about Voting Technology

Before turning to West Viriginia, I first present new evidence on public opinion related to online voting. In September of 2018, I partnered with The Associated Press and the NORC Center for Public Affairs Research to survey voting-age Americans about their views on voting technology. The survey utilized AmeriSpeak®, the probability-based panel of NORC at the University of Chicago. NORC devotes significant effort and resources to identify a representative sample of voting age Americans and maximize response rates. See Dennis (2019) for more details. Online and telephone interviews using landlines and cell phones were conducted on the topic of voting technology with 1,059 voting-age Americans.

We first asked respondents how likely they were to vote in the upcoming 2018 midterm elections, giving them the option to report that they will certainly, probably, probably not, or certainly not vote, along with the intermediate option of reporting that they are not sure if they will or will not vote. We then asked them to similarly report their intention to vote in the hypothetical scenario in which different voting technologies were their only option available. The various options were electronic ballots with and without a paper receipt, paper ballots that are scanned, paper ballots that are hand counted, ballots cast via mail, and ballots cast online.

Table 1 reports the survey-weighted proportion of respondents selecting each possible level of vote intention for each possible voting technology. The "avg" column reports the average intention to vote for each technology, where the vote intentions for each person are scaled such that "certainly not" is 0, "certainly" is 1, and the other options are evenly spaced in between. The "diff" column reports the difference in the average index for each voting technology and the baseline level of intention to vote. And the "p-val" column reports the two-sided p-value arising from testing the null hypothesis of no difference.

We see the highest reported intention to vote under electronic ballots with paper receipts and paper ballots that are scanned are a close second. Perhaps reassuringly, this is consistent with the recommendations of cyber-security experts who believe that a verifiable paper trail is the best way to protect the integrity of elections and guard against hackers who would like to manipulate election results (e.g., Blaze et al. 2018). For all the other technologies, voters are notably less likely to report that they would vote compared to the baseline vote intention question, and the differences are highly statistically significant. The lowest reported intention to vote is under online voting, with respondents 16 percentage points less likely to report that they will certainly vote under online voting than under their existing voting technology and with the weighted index being .122 points lower. Mail ballots are also unpopular, with the weighted index .087 points lower than the baseline. Studies of mail ballots in real elections show mixed results (Gerber, Huber, and Hill 2013; Kousser and Mullin 2007), and this finding is somewhat surprising in light of that evidence. It's possible that mail ballots have been successful in increasing turnout in places like Washington but they might have negative effects if introduced nationwide. Or it's possible that respondents underestimate their utilization of mail ballots if that were their only option. In any case, potential voters report that they would be less likely to participate under less standard technologies like online and mail voting.

We can also ask, descriptively, which kinds of people are more willing to participate under various voting technologies? In Table 2, I regress each respondent's vote intention (using the 0-1 scale described above) on binary indicators for age categories, gender, white racial identity, and college completion, along with the natural logarithm of reported income. Each respondent is weighted according to the survey weights, with the goal of uncovering the average relationships of interest for a nationally representative sample. I also include fixed effects for each possible vote intention response in the baseline question. For many of the voting technologies, most of the estimated coefficients are close to zero, meaning that the kinds of people willing to vote under this technology are not meaningfully different from the kinds of people who are generally willing to vote in the baseline question. Wealthier and college-educated people are more willing to vote with paper ballots that are hand counted, mail ballots, and online ballots. And people over the age of 50 are much less likely to report a willingness to vote online. So combining the results from Tables 1 and 2, we see that online voting is the least popular voting technology and this is especially true among older people, lower-income people, and those without a college degree.

I also embedded a randomized experiment into the question about online voting in order to study the conditions under which people might be more or less willing to cast their ballots online. Specifically, a randomly selected half of respondents were asked about their intention to vote if their only option was "votes cast online." And the other half were asked about "votes cast online using blockchain, the technology behind Bitcoin and other virtual currencies." Perhaps the biggest concern with online voting is the security of the vote totals, and advocates of online voting argue that these concerns can be mitigated through blockchain technology, so I wanted to test whether respondents find this argument to be reassuring or not.

To estimate the effect of this information, I regress willingness to vote online (as measured by the 0-1 index) on an indicator for whether the question was asked with or without the blockchain framing. As before, I utilize survey weights, and I show the results with and without fixed effects for baseline vote intention. These fixed effects are not necessary to obtain unbiased results, but they improve precision. Table 3 shows that telling respondents that online voting will be secured by blockchain technology paradoxically reduces willingness to vote online. The estimated effects are substantively large—.097 and .075 points on the 0-1 scale—and highly statistically significant. A plausible explanation for this counterintuitive result is that many people are not reassured by the presence of the blockchain framing, and furthermore, many people who were not thinking about security concerns are reminded by the blockchain treatment that online voting comes with increased security risks.

The survey data gives us reasons to be concerned about the effectiveness of online voting in improving participation. On average, people report less willingness to cast ballots online relative to other methods, and this is especially pronounced among less educated and lower income individuals who are less represented at the polls. Furthermore, the negative reactions to online voting are only worsened when we remind them of the security concerns by discussing blockchain technology. Now, we turn to West Virginia to estimate the effects of mobile voting on participation in a real election.

Mobile Voting in West Virginia in 2018

In 2018, West Virginia partnered with Voatz Inc. to offer mobile voting as an option for some of its overseas voters. They started with a "test pilot" in 2 counties (Harrison and Monongalia) during the May primary election. The test pilot was not advertised widely, and only 13 votes were cast using a mobile device, but state officials decided to expand mobile voting for the November general election. West Virginia did not want to excessively burden county election officials with this new technology, so they allowed counties to opt in or out of this project, and ultimately 24 of the state's 55 counties allowed mobile voting for overseas citizens. Figure 1 shows a map of West Virginia's counties, with the counties allowing mobile voting darkened. In total, 144 votes were cast with a mobile device in the November election.[3]

Mobile voting worked in the following way. As in every state, eligible voters in West Virginia who were living outside the country were eligible to submit a Federal Post Card Application (FPCA),

[3] See sos.wv.gov/news/Pages/09-20-2018-A.aspx and sos.wv.gov/news/Pages/11-16-2018-A.aspx for more information.

according to Uniformed and Overseas Citizens Absentee Voting Act (UOCAVA). Most of these citizens are active military personnel deployed overseas and their spouses, but other citizens living overseas are also eligible. If an FPCA is submitted and approved, the voter receives an absentee ballot along with instructions for submitting it. Overseas voters typically have the option of submitting their ballots via mail, fax, or scanning and emailing, and if the voters' permanent residence was in one of the 24 counties allowing mobile voting, they were also given the option to download a mobile app created by Voatz and submit their ballot online using the app.

The Voatz app uses the camera on the voter's mobile device to verify their identity, and it utilizes blockchain technology with the goal of protecting the anonymity and security of each individual's vote. While downloading a new app, verifying one's identity, and learning how to use a new technology is not costless, mobile voting is potentially more convenient and less time consuming that voting by mail or at a polling location. Among the 183 people who requested the app, 160 downloaded it, and 144 cast their votes with it.

Potentially limiting the effectiveness of mobile voting in this case is the fact that eligible voters first had to submit an FPCA through the mail in order to later vote using the mobile app. The kinds of people who are willing to submit an FPCA clearly do not find it too burdensome to fill out a paper form and submit it through the mail, so we might think that the effect of mobile voting will be lower among this subpopulation. The effects of mobile voting might be notably greater if registered voters could use the mobile app without first having to request it via mail.

To assess the effects of mobile voting on participation, I have obtained individual-level administrative data from the West Virginia Secretary of State's Office. This data contains information on every registered voter in West Virginia including their gender, age, party registration, county of residence, and turnout history. Ideally, we'd like to have data on all individuals who were eligible for UOCAVA status and test whether having mobile voting as an option made them more likely to vote.

Unfortunately, there is no database of individuals eligible for UOCAVA status, and we only learn that a voter is eligible after they submit an FPCA. Furthermore, we don't want to focus only on the sample of individuals who submitted an FPCA since the option of mobile voting may have increased FPCAs—a hypothesis that I later test and confirm.

Therefore, to identify a sample of individuals that are most plausibly eligible for UOCAVA status and who are likely comparable between counties with and without mobile voting, I identify all registered voters who previously submitted an FPCA for the primary or general elections in 2014 or 2016 or for the primary election in 2018. In other words, if an individual submitted an FPCA in any of the previous five elections for federal office, they are included in the sample, regardless of whether they submitted an FPCA in the 2018 general election. I also drop any individual who cast a vote in the 2018 general election without submitting an FPCA since these individuals were not UOCAVA eligible and since the availability of mobile voting for UOCAVA voters in their county likely had no effect on their decision to turn out. To be conservative, I drop the two counties that allowed mobile voting in the 2018 primary election, since the pilot may have affected FPCA applications in those counties in the primary, although the subsequent results are not meaningfully changed if I include those 2 counties and/or if I do not use FPCAs from the 2018 primary when constructing the sample.

In total, this sample consists of 1,754 registered voters in West Virginia who were likely to be living overseas and to be eligible for UOCAVA status. To be sure, some of the individuals may have no longer been living overseas in November 2018, and there are others who were eligible for UOCAVA status who are excluded from this sample because they either hadn't submitted an FPCA before or they only recently moved overseas. Among the 1.1 million registered voters in West Virginia not in my sample, only 293 (0.03%) submitted an FPCA for the 2018 general election. Conversely, among the 1,754 individuals in my sample, 397 (22.6%) did so.

To estimate the effect of a mobile voting option, I test whether an outcome of interest—either submitting an FPCA or casting a vote—changed differentially in November 2018 for the counties offering mobile voting versus the other counties in West Virginia. Specifically, I regress the outcome of interest in the 2018 general election on an indicator for the availability of mobile voting and controls for the outcome of interest in previous elections. In some specifications, I include controls for party registration, gender, and age. Although my sample includes 1,753 registered voters, the treatment of interest is clustered at the county level, and there could be county-specific shocks to participation, so my standard errors are clustered at the county level.

Because I control for prior patterns of the outcome of interest, and because mobile voting was first introduced in this sample for the 2018 general election and only for some counties, this is implicitly a differences-in-differences design. We are comparing changes in an outcome of interest for voters who did and did not gain the option of mobile voting. When studying turnout, I also run specifications that control for the level of turnout in various elections for voters who are not likely UOCAVA eligible. These specifications are akin to a triple differences design whereby we're implementing differences-in-differences designs separately for those who are and aren't UOCAVA eligible and then testing for a difference between the two. This design accounts for the potential concern that the counties participating in the mobile voting trial were experiencing general upticks in turnout in the 2018 general election, and it arguably makes an even weaker identifying assumption.

To assess the plausibility of my identifying assumptions, I conduct placebo tests in which I implement the same strategies for prior elections, before mobile voting was an option. Not all of the placebo results are statistically null, suggesting that the counties allowing mobile voting may have been experiencing differential trends in participation. However, most of the placebo estimates are null, and most are smaller than the actual estimates, suggesting that the biases are likely to be substantively small. But the placebo tests also suggest that we should be cautious in drawing overly strong conclusions about the effects of mobile voting from this one pilot in which the counties opting in may be unrepresentative.

Estimates of the effect of mobile voting on FPCAs are shown in Table 4. In Column 1, I control for whether each individual submitted an FPCA in each of the 5 previous elections. In Column 2, I add in controls for party, gender, and age category. In Column 3, I include fixed effects for each possible combination of FPCA history across the 5 previous elections, effectively matching individuals on their exact histories. And in Column 4, I include fixed effects for each unique combination of FPCA history, party, gender, and age category, effectively conducting exact matching on those covariates. Across all four specifications, the estimated effect of mobile voting on FPCAs is substantively large and statistically significant. Having mobile voting as an option in one's county makes a UOCAVA-eligible voter 6 to 9 percentage points more likely to submit an FPCA. Approximately 14 percent of the individuals in our sample in counties without mobile voting submitted an FPCA for the 2018 general elections, so this estimated effect represents a large increase over that baseline. Apparently, the prospect of being able to vote with a mobile app piqued the interests of many and led to a significant increase in FPCAs.

Table 5 shows the estimated effects of mobile voting on turnout. Column 1-2 and 4-5 are analogous to the specifications shown in Table 4, but with FPCAs replaced by turnout. In Columns 3 and 6, I also include controls for the county turnout rate among those not in the sample in each of the six elections. Not all of the estimates are statistically significant at conventional levels, although the estimated effect of mobile voting on turnout ranges from 3 to 5 percentage points. Substantively, this effect is larger than most get-out-the-vote interventions and most other electoral reforms including early voting and vote by mail.

Although the estimated effect of mobile voting on turnout is substantively meaningful, it is notably smaller than the estimated effect on FPCAs. This is consistent with the survey findings that many individuals are wary of online voting. Many people may have submitted an FPCA because they were curious about mobile voting, but when it came time to cast their votes, many of them did not follow through.

There are 1,148 individuals in the sample from counties that opted into mobile voting. If we suppose the effect of mobile voting on turnout was 4 percentage points (roughly averaging across estimates from Table 5), this suggests that 46 individuals voted who would have not otherwise voted if mobile voting wasn't an option. In the counties being studied (excluding Harrison and Monongalia), 107 individuals actually cast a ballot using the mobile app. This suggests that just over half the people using the mobile app would have voted anyway had the app not been available, but almot half the people using the app were induced to vote because of mobile voting.

Tables 6 and 7 show the results of the placebo tests. When studying FPCAs in previous elections, none of the estimates are statistically significant, although some are positive but with large standard errors. None of the placebo estimates are as large as even the smallest estimate in Table 4, suggesting that the estimated effect of mobile voting on FPCA applications is not easily attributable to differential trends of the counties opting into the trial. When studying turnout, however, some of the estimates are large and statistically significant, suggesting that we should be more cautious in interpreting the estimated effects on turnout. Because larger and more urban counties were more likely to opt into mobile voting, differential trends in turnout are possible. However, I have attempted to account for these differences as best as possible by matching voters based on their turnout histories, party registration, gender, and ages, and also controlling for general trends in turnout across counties.

Conclusion

Although many voters are understandably wary of online and mobile voting, when they actually have the opportunity to cast a vote online, many of them take it up, and a meaningful share of eligible voters are induced to vote who would not have otherwise cast a ballot. Although West Virginia's trial was small, only affecting overseas residents from some counties, and requiring individuals to first submit a Federal Post Card Application before utilizing mobile voting, the results suggest that mobile voting is more effective in increasing turnout than many other electoral reforms. Furthermore, if mobile voting were advertised on a larger scale and did not require the submission of mail applications first, presumably, the effects would be even greater. At the same time, mobile voting raises new security risks that should be closely considered before being further adopted. More states and localities are likely to experiment with mobile voting in the near future, and researchers should pay close attention to both the promises and perils of mobile voting when they do.

References

Bechtel, Michael M., Dominik Hangartner, and Lukas Schmid. 2015. Does Compulsory Voting Increase Support for Leftist Policy? *American Journal of Political Science* 60(3):752-767.

Berinsky, Adam J. 2005. The Perverse Consequences of Electoral Reform in the United States. *American Politics Research* 33(4):471-491.

Burden, Barry C., David T. Canon, Kenneth R. Mayer, and Donald P. Moynihan. 2014. Election Laws, Mobilization, and Turnout: The Unanticipated Consequences of Election Reform. *American Journal of Political Science* 58(1):95-109.

Enos, Ryan D., Anthony Fowler, and Lynn Vavreck. 2014. Increasing Inequality: The Effect of GOTV Mobilization on the Composition of the Electorate. *Journal of Politics* 76(1):273-288.

Blaze, Matt, Jake Braun, Harri Hursti, David Jefferson, Margaret MacAlpine, and Jeff Moss. 2018. Report on Cyber Vulnerabilities in U.S. Election Equipment, Databases, and Infrastructure. Report of the DEF CON 26 Voting Village <bit.ly/2Fj3NUq>.

Dennis, J. Michael. 2019. Technical Overview of the Amerispeak® Panel, NORC'Ss Probability-based Household Panel <bit.ly/2Ks32gf>.

Fowler, Anthony. 2013. Electoral and Policy Consequences of Voter Turnout: Evidence from Compulsory Voting in Australia. *Quarterly Journal of Political Science* 8(2):159-182.

Fowler, Anthony. 2015. Regular Voters, Marginal Voters, and the Electoral Effects of Turnout. *Political Science Research and Methods* 3(2):205-219.

Fowler, Anthony. 2017. Does Voter Preregistration Increase Youth Participation? *Election Law Journal* 16(4):485-494.

Gerber, Alan S., Gregory A. Huber, and Seth J. Hill. 2013. Identifying the Effect of All-Mail Elections on Turnout: Staggered Reform in the Evergreen State. *Political Science Research and Methods* 1(1):91-116.

Germann, Micha and Uwe Serdult. 2017. Internet Voting and Turnout: Evidence from Switzerland. *Electoral Studies* 47:1-12.

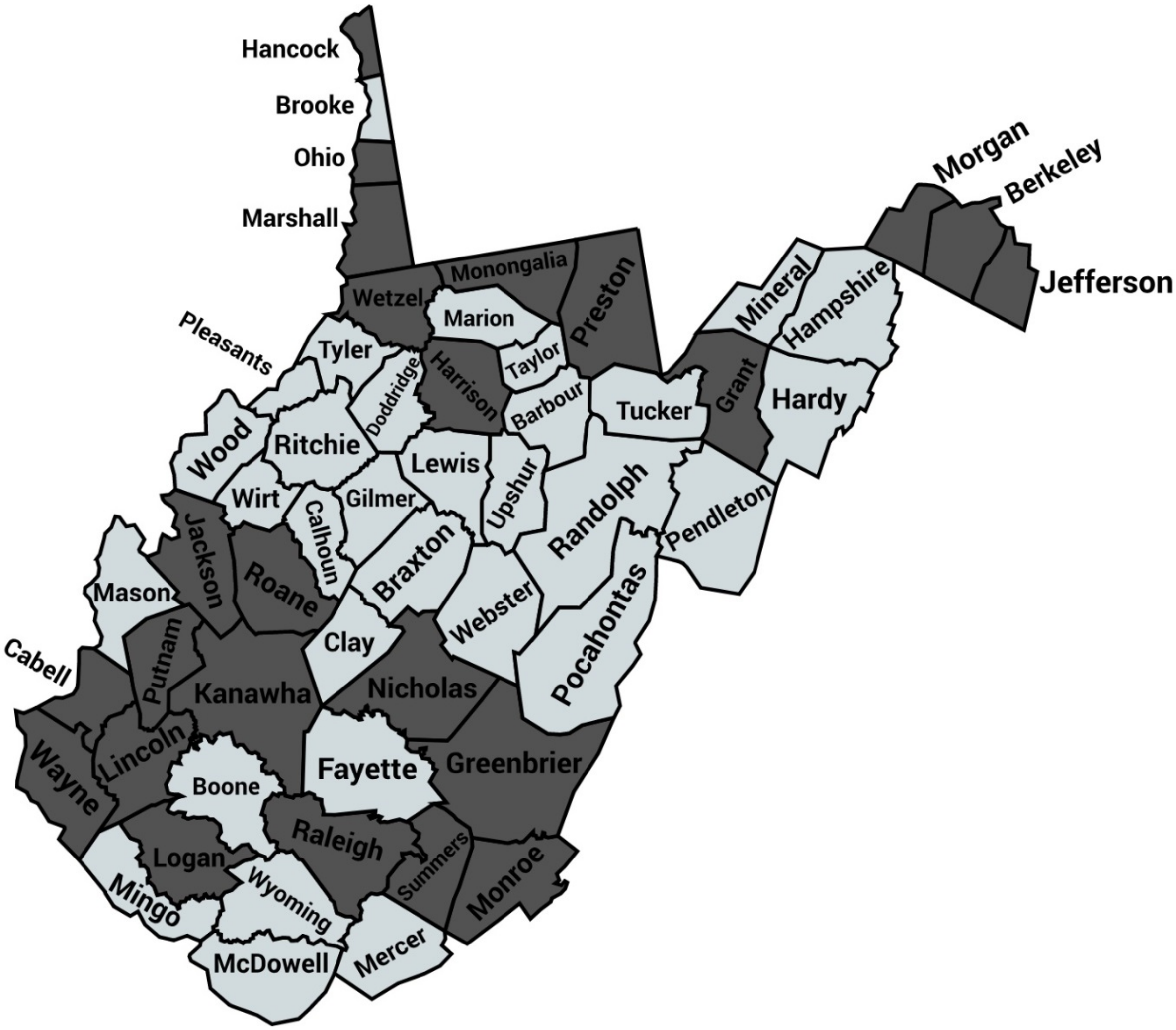
Goodman, Nicole and Leah C. Stokes. 2018. Reducing the Cost of Voting: An Evaluation of Internet Voting's Effect on Turnout. *British Journal of Political Science*. Advance online publication. doi:10.1017/S0007123417000849.

Green, Donald P. and Alan S. Gerber. 2015. *Get Out the Vote: How to Increase Voter Turnout*, 3rd edition. Brookings Institution Press, Washington, DC.

Kousser, Thad and Megan Mullin. 2007. Does Voting by Mail Increase Participation? Using Matching to Analyze a Natural Experiment. *Political Analysis* 15(4):428-445.

Lijphart, Arend. 1997. Unequal Participation: Democracy's Unresolved Dilemma. *American Political Science Review* 91(1):1-14.

Figure 1. West Virginia Counties Allowing Mobile Voting



Counties allowing mobile voting are darkened.

Table 1. Reported Intention to Vote with Different Technologies

	certainly not	prob not	not sure	prob	certainly	avg	diff	p-val
Baseline	.076	.061	.135	.161	.567	.770		
Electronic w/o paper receipt	.090	.076	.182	.201	.450	.711	−.059	.000
Electronic w/ paper receipt	.071	.042	.136	.229	.522	.772	.002	.839
Paper ballots, scanned	.074	.053	.163	.211	.499	.753	−.018	.059
Paper ballots, hand counted	.099	.069	.196	.182	.455	.706	−.064	.000
Paper ballots, mailed in	.103	.115	.174	.162	.447	.683	−.087	.000
Votes cast online	.114	.141	.191	.148	.407	.648	−.122	.000

The top row indicates the survey-weighted proportion of respondents giving each of 5 possible answers to their likelihood of voting in 2018. Avg *is the average of an index measuring intention to vote, with* certainly not *corresponding to 0*, certainly *corresponding to 1, and the other responses equally spaced in between. The subsequent rows report the comparable numbers when respondents were asked if they would vote if a particular voting technology was the only option available to them.* Diff *reports the difference in the vote intention index between each technology and the baseline question.* P-val *is the two-sided p-value arising from testing the null hypothesis that average vote intention is the same with this technology and in the baseline question.*

Table 2. Who Is More Willing to Vote with Various Technologies?

	electronic, no receipt	electronic, receipt	paper, scanned	paper, hand counted	mail	online
	(1)	(2)	(3)	(4)	(5)	(6)
Age 30-39	.009	.000	.003	−.006	.035	−.052
	(.035)	(.029)	(.031)	(.033)	(.035)	(.039)
Age 40-49	.022	.010	.034	.034	.010	.022
	(.028)	(.025)	(.024)	(.029)	(.026)	(.031)
Age 50-69	−.018	−.004	−.036	.021	−.017	−.061*
	(.025)	(.021)	(.021)	(.024)	(.026)	(.029)
Age > 70	−.053	−.005	−.024	−.032	.015	−.134**
	(.027)	(.020)	(.021)	(.024)	(.032)	(.041)
Female	−.032	.016	−.010	−.028	−.010	−.018
	(.018)	(.016)	(.016)	(.018)	(.021)	(.023)
White	.037	.004	.007	.010	.004	.010
	(.021)	(.017)	(.019)	(.020)	(.023)	(.026)
College	.036	.002	.020	.059**	.071**	.106**
	(.018)	(.016)	(.015)	(.017)	(.020)	(.024)
log(Income)	.018	.021*	.015	.030**	.050**	.049**
	(.010)	(.010)	(.009)	(.011)	(.014)	(.014)
Survey Weights	X	X	X	X	X	X
Vote Intention FEs	X	X	X	X	X	X
Observations	1,059	1,059	1,059	1,059	1,059	1,059

*Robust standard errors in parentheses; ** p<.01, * p<.05*

Table 3. Blockchain Framing Makes People Less Willing to Vote Online

	DV = Willingness to Vote Online	
	(1)	(2)
Blockchain Treatment	−.097**	−.075**
	(.028)	(.024)
Constant	.698**	
	(.019)	
Survey Weights	X	X
Vote Intention Fixed Effects		X
Observations	1,059	1,059

*Robust standard errors in parentheses; ** p<.01, * p<.05*

Table 4. Effect of Mobile Voting Option on Federal Post Card Applications

	DV = FPCA G2018			
	(1)	(2)	(3)	(4)
Mobile Voting Available	.088**	.073**	.085**	.063**
	(.023)	(.021)	(.023)	(.023)
FPCA P2018	.740**	.730**		
	(.041)	(.041)		
FPCA G2016	.161**	.151**		
	(.032)	(.035)		
FPCA P2016	.125**	.126**		
	(.018)	(.018)		
FPCA G2014	.128**	.109**		
	(.033)	(.032)		
FPCA P2014	.016	−.005		
	(.030)	(.034)		
Registered Democrat		.041		
		(.021)		
Registered Republican		−.045		
		(.024)		
Female		.018		
		(.017)		
Age 30-39		.053**		
		(.015)		
Age 40-49		.059*		
		(.022)		
Age 50-69		.172**		
		(.025)		
Age ≥ 70		.111*		
		(.044)		
Constant	−.092**	−.130**		
	(.032)	(.037)		
FPCA History FEs			X	
Exact Matching				X
Observations	1,754	1,754	1,754	1,754

*County-clustered standard errors in parentheses; ** p<0.01, * p<0.05.*

Table 5. Effect of Mobile Voting Option on Turnout

	DV = Voted G2018					
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Voting Available	.050*	.039	.031	.048*	.028	.034
	(.022)	(.020)	(.022)	(.022)	(.018)	(.021)
Voted P2018	.450**	.436**	.430**			
	(.043)	(.049)	(.049)			
Voted G2016	.094**	.078**	.074**			
	(.014)	(.013)	(.013)			
Voted P2016	.073**	.078**	.081**			
	(.022)	(.022)	(.021)			
Voted G2014	.117**	.099**	.097**			
	(.025)	(.025)	(.024)			
Voted P2014	−.051	−.060	−.042			
	(.055)	(.057)	(.056)			
Registered Democrat		.031	.036*			
		(.017)	(.017)			
Registered Republican		−.063**	−.062**			
		(.018)	(.018)			
Female		.004	.003			
		(.015)	(.014)			
Age 30-39		.075**	.076**			
		(.019)	(.018)			
Age 40-49		.096**	.098**			
		(.021)	(.022)			
Age 50-69		.159**	.157**			
		(.031)	(.031)			
Age ≥ 70		.089	.098			
		(.055)	(.053)			
County Turnout G2014			.467			.572
			(.507)			(.491)
County Turnout G2016			−.158			.217
			(.479)			(.606)
County Turnout G2018			.498			−.151
			(.485)			(.616)
County Turnout P2014			−.377			−.336
			(.262)			(.281)
County Turnout P2016			−.113			−.183
			(.417)			(.453)
County Turnout P2018			−.024			.180
			(.365)			(.401)
Constant	.012	−.023	−.214			
	(.016)	(.021)	(.117)			
FPCA History FEs				X		
Exact Matching					X	X
Observations	1,754	1,754	1,754	1,754	1,754	1,754

*County-clustered standard errors in parentheses; ** p<0.01, * p<0.05.*

Table 6. Placebo Tests for Federal Post Card Applications

	FPCA P2018		FPCA G2016		FPCA P2016	
	(1)	(2)	(3)	(4)	(5)	(6)
Mobile Voting (in G2018)	.054	.051	.038	.032	.013	.015
	(.035)	(.034)	(.035)	(.032)	(.028)	(.027)
FPCA G2016	−.091	−.094				
	(.051)	(.051)				
FPCA P2016	.067*	.067*	.051	.048		
	(.028)	(.030)	(.047)	(.046)		
FPCA G2014	−.020	−.023	−.320**	−.324**	.047	.032
	(.023)	(.024)	(.064)	(.060)	(.066)	(.065)
FPCA P2014	.031	.028	−.198*	−.204*	.046	.037
	(.068)	(.066)	(.098)	(.093)	(.091)	(.090)
Registered Democrat		.009		−.018		.116**
		(.023)		(.022)		(.034)
Registered Republican		−.019		−.015		.098**
		(.021)		(.023)		(.030)
Female		.009		.038*		−.039*
		(.012)		(.014)		(.018)
Age 30-39		.005		.044*		.045
		(.018)		(.020)		(.031)
Age 40-49		.011		.083**		.107**
		(.015)		(.026)		(.032)
Age 50-69		.017		.083**		.026
		(.023)		(.026)		(.031)
Age ≥ 70		−.004		−.157**		.001
		(.037)		(.056)		(.051)
Constant	.113	.113	.882**	.847**	.260**	.158**
	(.058)	(.059)	(.032)	(.031)	(.029)	(.031)
Observations	1,754	1,754	1,754	1,754	1,754	1,754

*County-clustered standard errors in parentheses; ** $p<0.01$, * $p<0.05$.*

Table 7. Placebo Tests for Turnout

	Voted P2018			Voted G2016			Voted P2016		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Mobile	.035*	.032*	.013	.067*	.063*	.061*	−.005	−.005	.021
	(.016)	(.015)	(.016)	(.026)	(.025)	(.027)	(.027)	(.026)	(.025)
G2016	.027*	.024*	.023*						
	(.011)	(.011)	(.011)						
P2016	.105**	.103**	.107**	.154**	.154**	.155**			
	(.019)	(.019)	(.019)	(.025)	(.023)	(.024)			
G2014	.036*	.030*	.027	−.026	−.051	−.054	.118**	.107**	.108**
	(.015)	(.014)	(.015)	(.034)	(.035)	(.035)	(.035)	(.036)	(.036)
P2014	.074*	.069*	.075*	−.122*	−.105	−.091	.243**	.227**	.213**
	(.031)	(.031)	(.031)	(.058)	(.058)	(.057)	(.046)	(.047)	(.049)
Reg Dem		.016	.018		−.008	−.003		.095**	.095**
		(.016)	(.016)		(.028)	(.028)		(.028)	(.027)
Reg Rep		−.005	−.005		.013	.013		.061	.059
		(.013)	(.013)		(.032)	(.031)		(.034)	(.034)
Female		.015	.014		.111**	.111**		.017	.018
		(.014)	(.014)		(.016)	(.015)		(.013)	(.014)
Age 30-39		−.007	−.004		.106**	.107**		−.083**	−.086**
		(.017)	(.018)		(.028)	(.029)		(.029)	(.030)
Age 40-49		−.007	−.004		.177**	.180**		−.008	−.017
		(.019)	(.020)		(.028)	(.028)		(.033)	(.034)
Age 50-69		.035	.034		.152**	.151**		−.049	−.047
		(.024)	(.024)		(.028)	(.028)		(.037)	(.038)
Age ≥ 70		.022	.031		.117*	.122*		.027	.019
		(.036)	(.035)		(.047)	(.050)		(.058)	(.060)
Cty P2018			.521						
			(.277)						
Cty G2016			−.151			.482			
			(.201)			(.325)			
Cty P2016			−.691*			−.348			.562
			(.277)			(.521)			(.381)
Cty G2014			.711**			.294			−.233
			(.260)			(.542)			(.461)
Cty P2014			−.290			−.126			.188
			(.171)			(.317)			(.285)
Constant	−.009	−.013	.013	.672**	.531**	.306	.192**	.169**	−.004
	(.014)	(.019)	(.089)	(.023)	(.035)	(.168)	(.023)	(.036)	(.094)
Obs	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754	1,754

*County-clustered standard errors in parentheses; ** p<0.01, * p<0.05.*